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ABSTRACT

This paper is part of a symposium focusing on the Safe School Study recently completed by the National Institute of Education. The symposium attempted to delineate the critical methodological problems arising from ethnographic research in the school setting on school violence and delinquency, and to report recent findings from studies using ethnographic methods conducted in several geographic settings. The author examines the appropriateness of ethnographic research for education and argues that positivistic designs do not establish interpretive understanding that is necessary to satisfy the duality of scientific proof. The Safe School Study is used as an example of the significance of qualitative research. (Author/MLF)

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# THE ETHNOGRAPHER IN THE SCHOOL: AN EXAMINATION OF EPISTEMOLOGY AND SCHOOL VIOLENCE

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## ABSTRACT

This paper examines the appropriatness of ethnographic research for education. It argues that positivistic designs do not establish interpretive understanding which is necessary to satisfy the duality of scientific proof. Prevalent myths are dispelled, and the Safe School Study is used as an example of the significance of qualitative research.

THE ETHNOGRAPHER IN THE SCHOOL: AN

EXAMINATION OF EPISTEMOLOGY AND SCHOOL VIOLENCE

Schools are funny places. They are obvious human creations with all attendant strengths and flaws. Nevertheless, their veracity and integrity are somehow historically proven by their survival even though we are well assured that no alternative was allowed to challenge this survival. To be blunt, we can conceive of the historical test of schools to indicate not their viability but their political prowess. Their initiation was politically, economically and technologically appropriate to the rising industrial order and the potential threat of immigrants and Catholics to the Protestant power elites (Katz 1971) who controlled the new industries.

Of course, this statement will, for some of you, serve as evidence that I am dogmatic and another one of those radicals who not only challenges schools but also the social and economic order of things. In part, it is this response that I wish to concern myself with during this presentation--for this response indicates a faith in what is even if an alternative has never been fully assessed. The faith I am concerned with today is the faith in positivistic research which, I will argue, is like all other methodologies simply a set of guidelines and rationales that will yield reasonably good data as long as its assumptions and limitations are inviolate. However, I will argue that it lacks full integrity as <u>the</u> methodology to achieve scientific proof since it cannot establish interpretive understanding of the quantitive associations it generates. In short, positivism alone will suffer from the "black box" problem, although a more

phenomenological methodology will not.

To develop this argument, a number of issues need resolved. First, we must clarify what is meant by a more phenomenological methodology by describing ethnography, the most common of phenomenological approaches. Second, we must understand more fully the duality of scientific proof. Third, we must explore and dispel some common myths about qualitative research. Finally, we will explore the Safe School Study as an example both of ethnographic and positivistic research and compare their respective potentials as policy research modalities.

## Ethnography and Observation

Before one can establish the significance of qualitative educational research, some attempt at definition and description is necessary. This need not be an elaborate endeavor. Rather, let me confine my remarks to distinguishing simple observation from ethnography--the major qualitative approach currently in use in educational research.

Observational strategies are commonly used in the study of educational programs. Unfortunately, it is the usual case that only "simple" observation is employed. By "simple" observation I mean that type of observation which is not treated as a formal research technique or which is restricted to only "counting" behaviors. Of course, all researchers use observational data, since it is the basis upon which the final research design is formulated. Further, it is used to establish a basic description

of the problem in question. However, "simple" observation is employed ina piecemeal fashion and used to quite limited ends as noted above. Even when it is used to establish the work patterns or motion patterns of the participants via "counting", the limitations placed upon observation by the principal investigator are evident.

Ethnography is not "simple" observation nor an expansion of simple observation, for it allows for an understanding of the complete setting, its components, and its historical process, and it does so in the terms of the meaning categories of the participants. That is, ethnography captures the essence of a setting, and the variety of essences, according to the categories of those who work in it, pass through it, or attempt to impact upon it. In regards to applied research, the ethnographic approach has been described as being emic, holistic, historical and comparative.

Spicer (1976) writes:

> In the study there should be use of the emic approach, that is, the gathering of data on attitudes and value orientations and social relations directly from the people engaged in the making of a given policy and those on whom the policy impinges. It should be holistic, that is, include placement of the policy decision in the context of the competing or coorperating interests, with their value orientations, out of which the policy formulation emerged; this requires relating it to the economic, political, and other contexts identifiable as relevant in the sociocultural systems. It should include historical study, that is, some diachronic acquaintance with the policy and policies giving rise to it. Finally, it should include consideration of conceivable alternatives and of how other varieties of this class of policy have been applied with what results, in short, comparative understanding (Spicer, 1976: 341).

Obviously, ethnography is more than an assessment of the impact of event upon some client group, for it would argue that such an assessment does not provide sufficient understanding of the nature of the event, its initiation, its historical underpinnigs and meanings; how those served and those serving conceive of the event, its meaning and its initiation; and how that event compares with other events, conceptions and patterns that are present in a school setting. In short, ethnography is not the inadequate approximation of a quantitive study, but rather the more complete analysis and synthesis than quantitative studies attempt to reductionalistically capture. Unfortunately, this reductionistic rendering is inadequate-- not necessarily because it has limited scope, for not all quantitative evaluations do. It is inadequate because it is insufficient for scientific proof inasmuch as it cannot establish such things as casualty.

The Duality of Scientific Proof

There has been much bantering over whether hypothetico-deduction or analytic induction is the true method of science. Znaniecki (1934) has argued that the latter is the true method of the natural sciences; Homans (1967) argues for the former. However, some philosophers of science, most notably Peter Winch, have attempted to portray the duality of a scientific proof. Most researchers would argue that, of course, qualitative and quantitative research are complementary and when used conjointly may serve as triangulation of results. This is not the duality that we are concerned with here. In fact, the above common

argument demonstrates a fundamental misunderstanding of the true duality of a scientific proof. The duality cannot be expressed as complementary, for one part of the duality is necessary to the other, while the reverse is not true. The common understanding that denotes the relationship as complementary reflects, in part, the dominance of the quantitative approach,

and in part, the inadequacies of the explanations of the logic of interpretation to which researchers have been exposed (Turner and Carr, 1976). Let us attempt to rectify the latter.

The works of Turner (1953), Bensman and Vidich (1960), Winch (1967), McCarthy (1973), and Turner and Carr (1976) all point to the duality as a necessity for fully adequate explanation of a social phenomena. The duality has been expressed alternatively as analytic induction and enumerative induction (Robinson, 1951), theoretical prediction and empirical prediction (Turner, 1953), heuristic and systematic theory (Bensman and Vidich, 1960), and interpretive understanding and causal explanation (Turner and Carr, 1976). The latter formulation seems to be the most adequate inasmuch as it is inclusive of the basic arguments of the others but seems to respect the duality most inasmuch as the others are either positivistic interpretations of the duality or more allowed the positivistic critiques to establish the parameters for discussion than have Turner and Carr. Further, Turner and Carr frame the argument in terms of the larger issue of criticism and theory development and address their arguments to one explanatory system and its critique from two disciplines, sociology and history. Thus, it appears that such a complete argument framed in interdisciplinary terms would be most appropriate for educational

research since it remains a highly interdisciplinary field.

Interpretive understanding is the qualitative component of the duality, while causal explanation is the quantitative, probablistic assessment. The former has been conceived as a "closed system" by Ralph Turner (1953). He argues that the application of analytic induction will produce a causally contained system, isolated by definition from intrusive factors that will activate the closed system of causal process. Boldly stated, interpretive understanding is, "placing the act in an intelligible and more inclusive context of meaning" (Weber, 1968:9). Thus, it is invariably attuned to the notion of intention in any action context. Interpretive understanding is that understanding that can in the context of any specified action system account for the meaning of the juxtaposition of events on some plane (i.e., time or space). Interpretation, then, is "an observation technique appropriate to particular kinds of facts... . If we view interpretation of meaning in (this) way, interpretative claims must be regarded as observational hypotheses, to be confirmed or disconfirmed by direct application of the technique" (Turner and Carr, 1976:4) Turner and Carr cite Weber for an account of the method:

> All interpretation of meaning, like all scientific observations, strives for clarity and verifiable accuracy of insight and comprehension (Evidenz). The basis for certainty in understanding can be either rational, which can be further subdivided into logical and Mathematical, or it can be of an emotionally empathic or artistically appreciative quality. Action is rationally evident chiefly when we obtain a completely clear intellectual grasp of the action-elements in their intended context of meaning. Empathic or appreciative accuracy is attained when, through sympathetic participation, we can adequately grasp the emotional context in which the action took place (Weber, 1968:5).

Interpretive understanding and causal explanation conjoin so that:

> ...we understand the motives of an individual which may be the cause of action, and our grounds for this 'understanding' is 'sympathic participation' or an 'intellectual grasp'. Explanation, however, is achieved only when we have identified the actual cause (Turner and Carr, 1976:6-7) (emphasis in original).

As such then, causation is possibly best a probability that is calculable but may not be numerical (that is, it may be Mills' "method of difference" where the largest number of processes that differ on one decisive point are compared). The probability is that one observable event, overt or subjective, will be followed by some other event (Weber, 1968:10-12). However, explanation requires an understanding of motivation and intention as found in the setting and some calculable probability.

Thus, it appears that causal adeqacy requires that both interpretive understanding and causal explanation be obtained:

> The causal interpretation, taken as a whole, is adeqate if and only if it is adequate on the level of meaning and on the level of established transition probabilities (Turner and Carr, 1976:7).

The duality of scientific proof has often been ignored by educational researchers like us. All too often, qualitative studies are seen as inadequate because they only generate hypotheses according to conventional logic, and because it is more fruitful for researchers to gather quantitative data so as to better establish causation. This type of logic belies the duality of scientific proof and has disastrous implications for how we proceed with research and the conclusions which we draw.

Myths, Promise and Peril

The tertiary status of ethnographic designs in educational research has left them open to many misinterpretations, which may, in fact, have the coherence of a complete mythology. Let us critique some of the myths. Myth 1: Qualitative research is less objective than quantitative. All of us obviously see a flaw in this statement. However, the flaw may be more than we usually assume. Objectivity is the result of the interaction of the researcher and the research technique(s). Often we argue that the researcher needs to be detached and impersonal to maintain objectivity, and his/her technique needs to not independently affect behavior. Phillips (1971) points out that it is rare that this is even true of survey research. Further, ethnography may be better equipped for objectivity than are quantitiative designs since its goal is to be "emic". It does not impose response sets or theoretical categories upon a set of respondents; it identifies the sets and categories as they exist in the situation. Further, ethnography requires that the data be exhausted in terms of confirming a heuristic or abductive hypotheses. If the data cannot be exhausted, then modifications in the hypothesis are made or a substitute hypothesis is formulated that better explains the data. As it turns out, the theory-free inquiry of the ethnographer should be at least equal in objectivity to the theory-laden inquiry of the quantitative researcher (Douglas 1976).

Myth 2: Causal explanation cannot be invoked by the ethnographer

(Smelser, 1976: 204). As we have demonstrated earlier in this paper, the ethnographer is certainly at no less handicap than is the quantitative researcher in making an adequate causal understanding. Even though probabilistic assessments are more difficult (but possible, see Glaser, 1965), it is ethnography that allows for a parsimonious social science. Its interpretive understanding reduces the number of possible hypotheses that an enumerative strategy can then translate into a causal explanation. Without that interpretive understanding, the elimination of alternative explanations for any study must be based on a consensually derived or "democratic", as some call it, definition of truth; that is, one that we have all come to agree upon, whether or not a particular study or set of studies truly confirms it. Further, since ethnographies typically take place over some extended period of time, temporal sequence is more easily established than with more cross-sectional designs.

Myth 3: Qualitative researchers are typically interested in the explanation of a single event (Smelser, 1976: 204). Again, Glaser's (1965) constant comparative method is one strategy to resolve this type of usual misunderstanding. However, this myth demonstrates an ignorance of ethnographic analysis. As one exhausts his/her data in developing a processual understanding, it also creates a distribution of events. Of course, it is true that an holistic orientation leads to attempts to synthesize rather than analyze data. Thus, ethnography produces an event,

quantitative studies produce distributions of events. In the case of education, the researcher requires both types of understanding, and particulary if causal judgments are to be made to guide program development. But most importantly, an ethnography will display an event or number of events as they actually appear and as they develop over time; quantitative studies produce the distributions in large part because they are designed to do so.

Myth 4: Qualitative studies do not exclude extraneous variables (Smelser, 1976: 204) Smelser (1976) argues that qualitative "modes of explanation typically incorporate different orders of causal variables.." and thus "...'accidental' factors are given play (Smelser, 1976: 204; emphasis in original)". Smelser misses the point that ethnographies synthesize the effects of variables at a variety of levels of analysis and thus demonstrate for any given situation, event or organization how these levels of analysis interact. Quantitative analysis rarely does this and almost without exception does not accomplish it with sufficient accuracy to guide educational program development, initiation or redirection. For them, "interaction" of variables is a statistical property, not the actual process. Logically,"accidental" factors are so judged from reviewing a mass of cases where those factors do not explain much variance. However, in education, the required approach is usually limited to a program, a school, a school system, (and if we are lucky, a comparison event). Therefore, if a factor affects a setting, it is not "accidental." It is significant to that setting. Rarely is adequate data availabe to judge with any certainty whether factors are "accidental" or not.

Myth 5: Comparison or control is obtained for qualitative studies only by alluding to other knowledge or an imaginary experiment (Smelser, 1976: 204). I could candidly argue the same is true of quasi-experimental designs, but rather, let me note that on a practical level qualitative comparisons are direly needed in education. The holistic approach would seem to enable an identification of crucial "variables" via Mills' method of difference, and not as a result of a conspiracy of researchers to measure a particular set of variables. Qualitative research will more closely permit the true variables to emerge.

As an aside, it should also be remembered that each case in a quantitative study is, in fact, a replication, but since it is practically impossible to guarantee that each replication took place under the same conditions, their comparability is difficult to assume (although we hope for random variation in the possible conditions and theri effects). Most of the comparability is guaranteed by the response set available on the data collection instruments.

Myth 6: Qualitative and quantitative studies do not differ with respect to the causal forces invoked (Smelser, 1976: 205). This myth has been perpetrated by apologists for qualtitative methods. Bruyn (1966) notes that qualitative researchers end up being less deterministic, since their involvement in the situation allows at least some degree of voluntarism of the participants to be observed. My own research specifies this even more (Collins and Noblit, 1976). In looking at the field of interracial education,

it was evident that qualitative studies were more likely to reveal the formal organization as a contingency with which the individual must interact. Quantitative studies in that same field represented a determinism that argued that the participants had personal problems that were causal. In short, there is a difference in types of conceptual frameworks employed. I would argue that the former, more phenomenological orientation needs to reach its full fruition before the latter is invoked in order to satisfy the duality of scientific proof.

Myth 7: Qualitative research cannot test hypotheses. Obviously, the corolary of this is that they only generate hypotheses. Of course, one way to critique this is to argue that it depends upon the type of hypotheses---abductive or deductive. It also appears that qualitative designs are essential to testing deductive hypotheses in the way we noted in the preceeding section of this paper. But, to be more pragmatic, ethnographies are better able than surveys to establish temporal order, and, thus may be able to better test some deductively derived hypotheses. Further, in the face of the massive amounts of quantiative research that have already been undertaken, it would appear that the hypotheses that have received some quantitative support should also be put to the ethnographic test. As Turner and Carr (1976) have suggested, only qualitative research can establish the meaning of a hypothesized relationship between variables. Given a probabilistic assessment, qualitative strategies can reveal the appropriate interpretation of any relationship.

Obviously, this discussion of myths implies that qualitative research does have some promise for the educational research and that the mythology concerning their limitations has clouded the examination of their potential. The promises lie in the emic, historical, holistic, and comparative assessments they allow. Further, Goode and Hatt (1952:333-334) note that the strengths of qualitative research lie in the breadth of data, the levels of data, the formation of indices and typologies, and the interaction in a time dimension that they allow. No other technique can match them in these respects. Finally, it also appears that these types of designs may be exceptional in regards to an issue that has continually plagued social researchers and evaluators--social change. Through qualitative designs the impact, meaning, and process of social change are best assessed, since change can be seen in its objective form and in the response of the organizational participants to it. The emic and historical nature of qualitative research provides categories and meanings of events in the eyes of the participants at different points in time and shows the transition between them. Educational research needs these kinds of data.

I would be remiss if I did not mention the peril of qualitative research. Its perils are much the same as any other research design. The main peril is that it may be misused. It is not the best design for all questions and alone cannot establish causality. Further, proper usage of the technique requires extensive training and experience that is not usually

imparted in graduate study in the social and behavioral sciences. Thus, a major peril is that a researcher may not shed his/her positivistic underpinnings (at least, for purposes of the method) and use it as an approximation of a quantitative approach. As a result, valuable data and interpretive understanding are both lost. This has one other implication also. The positivistic researcher may not be true to his/her observations, and thus, he/she may reify any deterministic notions he/she holds. Of course, survey instruments and analyses have the same problem, but they are more firmly rooted in positivism. This leads to the last peril. Survey researchers usually report the questions which are used to operationalize concepts. Thus, the questions are subject to review and criticism. An analogous procedure has yet to be fully developed for qualitative designs, although descriptions of how classes of events and linkages were constructed may serve to alleviate some of this peril.

Even with thses limitations, however, it should be noted that a number of private researchers and Federal research funders have told me that qualitative research is receiving increasing play and will continue to do so. This, it appears, is the result of increasing reliance on contracts as opposed to grants for research and evaluation relevant to policy decisions, and the increasing scrutiny of data gathering instruments used most commonly in quantitative research by the Office of Management and the Budget. These two developments interact to create "deadtime" as the research team awaits approval which must be paid for by the funding agency. This quantitative research is quickly becoming more costly, time consuming and political. As

a result, it would appear to be in the intellectual (if not political and economic) interests of researchers to truly understand the place and procedure of qualitative methods in social research.

The Safe School Study

With all that discussion behind us, let us look a little more closely at the Safe School Study as an example of the differences between the approaches. It involved three phases. Phase I was a mail survey of 4,000 elementary and secondary school principals which asked them to report incidence over one month reporting periods. In Phase II, an on site survey was conducted of a nationally representative cluster sample of 642 junior and senior high schools in which principals again recorded incidents for a month and supplied other details about their school, student and teachers were surveyed about their victimization in that month and provided other data about themselves, their schools and their communities. Phase III was the ethnographic study of ten schools by six researchers. Phases I and II yielded assessments of incidence and victimization of students and teachers by level and location, perceptions of change, relative seriousness by elementary, junior high and senior high schools and by urban, suburban and rural, costs, relative risks by region of the country, reports to police, time and place of incidents, influence of neighborhood crime levels, effects of racial composition, effects of family, school size, effects of incentive structures, security devices and personnel, disciplinary procedures, recommendations on ways to reduce school crime and other variables. The Phase III ethnographies yielded five major sets of findings on : 1) the relationship between schools and their communities

and definition of school vandalism and violence : 2) the significance of school size and design in violence and vandalism; 3) The factors of student and staff identification with the school, the composition of the school population and the quality of student life as aspects of the climate of the achool; 4) the factors of leadership, staff relationships, authority, and discipline and rule structures in the responsiveness of the school climate to problems of vandalism and violence, and (5) the special problem of the relationship between making schools safe and maintaining the quality of their educational functions.

Comparing these two lists, we see many differences. On first impression, the quantitative study seeemingly examined more factors. This appears to be so, and probably is, for a number of reasons. First, the level of effort in the first two phases by design was much much greater than in the third phase. Second, surveys are designed to give discrete data and variables while ethnographies are designed to be more holistic. Thus the differences in the number of variables is in part, shrouded since the holistic orientation synthesizes them more into the fuller context of their existence and meaninf. Third, the first two phases were asked to establish incidence and victimization rates, the ethnographies were not. Therefore, the differences are not as dramatic as they first seem. However, it should be noted that the ethnographers (who were deployed after the quantitative data had been gathered) found good reasons not to trust the incidence data. In some cases the principal who was asked to complete the forms actually did not, in some cases where he/she did complete the forms others such as

vice-principals had better data on incidence, and in general we found that hte perceptions of the exact nature and seriousness of offense varied by the level of crime in the surrounding community so that the reporting would vary in its accuracy. In high crime areas and schools, it was probably low because many offenses were ignored as insignificant, and in low crime areas and schools they were probably high because even disruptions were treated as significant. Many cases, the ethnographers found that student misbehavior and disrespect were causing more disruption to the school that were most acts of violence or vandalism.

Again returning to the comparison of the variable lists of each approach, quantitative and qualitative, we note that if incidence variables are eliminated from the list that quantitative study variables reflect (with the exception of the recommendations) standard explanatory variables for variations in crime rates. These explanatory variables were derived from theory and past research as positivism requires. The synthetic five sets of findings of the ethnographers came only from their setting being so studied.

This difference is crucial particularly when addressing policy research such as this and the majority of educational research as well. The differing epistemologies that governed the data collection in the quantitative phases and the qualitative phase of the study has direct implications for the utility of the research. While obviously I am pushing the point somewhat, it appears logically that the quantitative study given its essentially deductive derivation serves theoretical interests. That is, given that

the quantitative variables are theoretically significant and have found some play in past research and that they were deductively derived from this research, it is obvious that the results best inform that theoretical and research base. In pursuit of pure research this is not usually regarded as problematic. However, if the case of policy research it is disasterous. Theory and policy rarely go together since by definition the latter has strong "practical" elements. The appropriate deductive reasoning for applied policy research would be to take possible policy options and from them distill variables to be tested. In that way, the interests of policy and policy makers could be more appropiately served. In either case, however, the deductive, positivistic model lacks a final utility. Its reductionism does not allow "new" variables to emerge. In either case, it is bound to that which it was designed to serve.

Ethnography, on the other hand, does not serve theoretical or policy interests in its design. It is limited by the setting only (and time and effort as is all research). It establishes the factors as they exist and places them in an interpretive context while the positivistic approach only speculates that context. As a result, in ethnography not only are crucial factors identified, but also their interactions are more fully understood, and their interpretations emically characterized from these data, both theory and policy can be informed. In fact, it was reported that the qualitative studies were much better received by a practitioner review group than were the quantitative results. While the practitioners were not in full agreement with the results of qualitative studies, they felt the ethnographies better captured

and depicted the problems and the contexts of these problems.

To be abrupt, the agrument is simply that the inductive, qualitative designs are simply more informative for public policy. Their complex data better elucidate possible tradeoffs in any specific policy initiative. That is, they can better assess how a policy would "fit" schools and the problems the policy is attempting to alleviate. Positivistic designs cannot do this, because their interests lie in theory or even policy. The interests of ethnography are to be true to the setting being studied.

Conclusions

Let ne clarify once again the intent of this paper. It was not to criticize the Safe School Study. I have great respect for those who conceived and conducted it. Given their mandate, funding, and time constraints, they did as well as they could--except they should have conducted the ethnographies first and based their survey instruments on them. The purpose of this paper was to examine quantitative positivism by a comparsion with its major competitor, an inductive, qualitative design called ethnography; to dispel some myths about the "adequacy" of positivism and the "inadequacy" of ethnography, and to challenge our conventional wisdom of what brand of science is the more appropriate or knowledge and particularly applied knowledge.

As noted earlier, we need a pluralistic definition of science if we are to fully satisfy the duality of scientific proof. Both quantitative and qualitative designs are required. Quantitative designs are good for incidence assessments and develop comparable estimates of the strength and significance of variables. However, ethnographies are better at identifying these variables and placing

them in their fuller contenxt of meaning. I guess my real gripe is that we have created social theory and now somehow feel bound to use it. It is sad when humans reify their creations.

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